to represent both on a skeleton map as constituting one great range or axis of elevation. The sub-Himalayas consist of rocks of different age from those of the Himalayas, and there is some reason for believing these hills to be of later origin than the main chain; they are therefore represented in our map as a distinct range.

It would take too much space to criticise at any length Mr. Trelawney Saunders's Tibeto-Himalayan system (*Geographical Magazine*, 1877, p. 173). This system proposes to resolve "the leading outlines of the vast mass of which it treats into four great chains, with their outer slopes and intermediate valleys or plateaus." The chains are called the Kuenluen, Karakorum-Gangri, and Northern and Southern Himalaya. Now the greater part of the Tibetan area, including, at all events, all east of the meridian of 82° E. long., is too imperfectly known for any positive assertion to be made as to the number of ranges. In the better known western part of the area one fact alone, the omission to include as one of the main structural features, the range between the Indus and Shayok, shows the description and delineation to be geographically incorrect. The range omitted is at least of equal importance with some of those included. There are many other points open to question, such as the representation of the ranges north and south of Cashmere, as mere continuations of the so-called Northern and Southern Himalaya. In short the system will not fit into the only part of the area with which we have any adequate acquaintance. The accompanying map is doubtless an admirable sketch of the Himalayas as they would be if reconstructed according to Mr. Trelawny Saunders's hypotheses, but I think all who have ever been in those mountains will agree with me that it is not an accurate representation of the range as at present existing.

In conclusion I must decline to reply to any further remarks on this subject from Mr. Trelawney Saunders. It appears to me that Mr. Medlicott and I are entitled to express an independent opinion on the physical geography of the Himalayas without being accused of adopting an antiquated theory. In addition to the geographical data known to Mr. Saunders we have some acquaintance, imperfect, it is true, but still of importance, with the geology, and we have both some slight personal knowledge of portions of the range. Under these circumstances we have not adopted the theory advocated by Mr. Saunders simply because we consider it not supported by sufficient evidence.

February 29 W. T. BLANFORD

[This correspondence must end here.—ED.]

Tidal Phenomenon in Lake Constance

LES mouvements de la glace et de l'eau du lac de Constance décrits par M. S. J. Capper (NATURE, vol. xxi. p. 397) ne doivent pas être rapportés à une marée luni-solaire, ce phénomène étant inappréciable sur un lac dans si petites dimensions. Je me fonde sur les résultats négatifs que j'ai obtenus sur le lac Léman, plus grand en longueur et en surface que le lac de Constance. En utilisant les tracés de mon limnographe de Morges qui me permet d'évaluer à chaque instant à un millimètre près, la hauteur du lac en choisissant les circonstances les plus favorables, calme absolu de l'eau, et époques de syzygie, je n'ai jamais pu reconnaître de traces de marées luni-solaires.

En revanche les mouvements de balancement de l'eau que nous étudions depuis bien des années sous le nom de *seiches*, expliqueraient facilement une partie des faits signalés. Les seiches, comme on le sait, sont un mouvement de balancement de toute la masse du lac, qui oscille d'une extrémité à l'autre comme le fait l'eau d'une cuvette ou d'une baignoire. Il est vrai que le rhythme des seiches du lac de Constance, pour autant que je le connais par une seule observation du 14 septembre 1874, n'a qu'une durée d'une heure environ, et non douze heures comme l'indique le batelier de M. Capper. Il serait fort à désirer, pour l'interprétation de phénomène, que M. Capper put fournir des données et observations aussi exactes que possible des mouvements qu'il décrit. F. A. FOREL

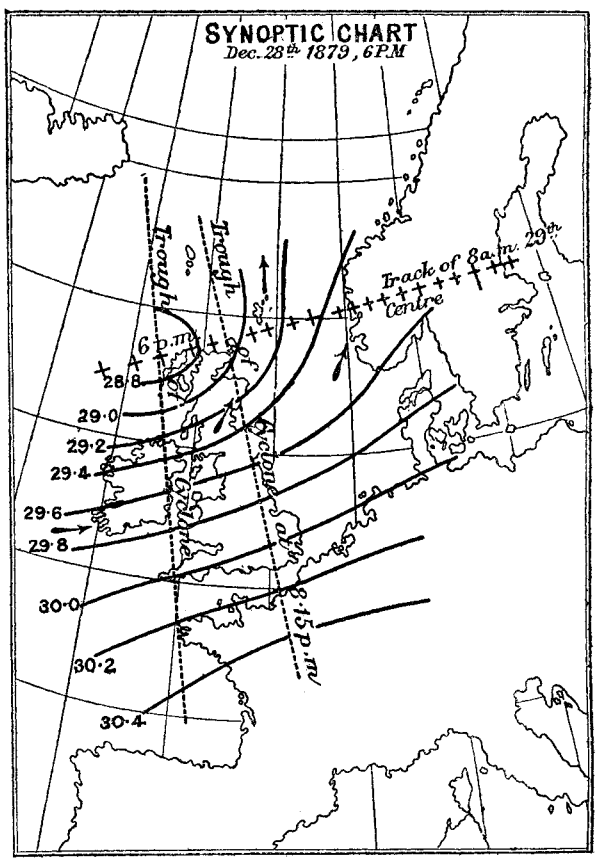
Morges (Suisse), 3 mars

The Tay Bridge Storm

A BRIEF account of the results obtained from the examination of a large number of observations referring to the storm on December 28, 1879, may be of interest even to your non-meteorological readers.

At 6 P.M. on the evening of that day, as will be seen by the accompanying chart, Fig. 1, the centre of a cyclone of considerable intensity was situated close to Stornoway. By 8 A.M., the 29th the centre had moved a distance of about 800 miles to the vicinity of Stockholm, which gives the high mean velocity of 58 miles an hour. But by a method detailed below, it is found that between 6 P.M. and 8·15 P.M. the centre moved along the north of Scotland at the rate of 62 miles an hour, which is, I believe, the highest on record in this country. No precise relation has yet been traced between the velocity of a cyclone centre and the strength of the wind in it. In any part of a cyclone the velocity of the wind is undoubtedly principally dependent on the closeness of the isobaric lines, but there is a good deal of evidence to show that when the velocity of the centre is very great, the strength of the wind for any given gradients is increased, or at all events becomes more squally and gusty.

In this case the steepest gradient was down the west of Scotland, but only amounted to about 0·13 inch per 50 miles, which is a very moderate amount for a winter storm.



An important result of recent research has been the discovery that every cyclone is divided into two parts by a line drawn through the centre, more or less at right angles to the direction of its motion, at all points in front of which the barometer is falling while it is rising in rear. This line marks out what is called the trough of a cyclone, and while the front and rear present marked contrasts both as regards the in-curvature of the wind, and still more as regards their physical appearance, it is also found that the passage of the actual trough all along its southern portion, except very near the centre, is marked by violent squalls. In the accompanying diagram the position of the trough at 6 P.M. can only be drawn approximately from general considerations as passing down the west of Scotland, but at 8.15 P.M. I have fortunately been able to locate it with great accuracy. At that time the barometer turned upwards at Wick, and almost at the same moment my own barograph in London also turned upwards with the characteristic squall. The line of the trough joining those two points would then be about thirty-three miles east of Dundee, and by combining it with the previous data, the high centre velocity of sixty-two miles an hour was obtained.

Turning now to Dundee, observations there show that the barometer fell very fast till about 7 P.M., after which it remained nearly stationary for about two hours; at 7.15 the Tay Bridge was blown down; about 7.45 the actual trough of the cyclone passed over the town, and about 9.30 P.M. the barometer began to rise. The wind, which had been strong all day, rose to a strong gale with violent gusts and squalls at 5 P.M., and lasted till 8.30, when the weather began to moderate.

Thus it would appear that in this storm at Dundee, as is often the case, the worst weather occurred just before the barometer ceased to fall, and during the two hours it remained nearly stationary previously to rising rapidly. The Tay Bridge was blown down by an ordinary gust during this time, and not in any squall during the time of the actual passage of the cyclone's trough.

On the whole it may be said that though the storm which destroyed the Tay Bridge was in many ways of the most ordinary character, it was exceptionally squally and gusty, doubtless owing to the unusually rapid rate of its motion.

One word in conclusion, as to the destructive effect of wind. A gust strikes with a blow, which can no more be calculated from the velocity of the wind, than the blow of a sledge-hammer can be estimated by a pressure in tons, or by the energy of so much *momentum*. But observation also shows that in squalls and gusts there is a great deal of local compression of the air; fluid pressure must then come into play, and in this we have, probably, the explanation of the remarkable lifting power of wind, which has been so often described in great storms. Unfortunately in our present state of knowledge, this lifting force is as incapable of numerical estimation as the lateral blow of the gusts.

My acknowledgments are due to Dr. Copeland for his courtesy in furnishing me with copies of the meteorological records of the Dunecht Observatory, situated about fifty-six miles north-north-east of Dundee.

RALPH ABERCROMBY

7, Royal Terrace, Folkestone

A Lecture Experiment on Ice-Crystals

THOUGH different processes are at command on the lecturer's cable to illustrate the artificial formation of ice, none of them may be said to yield the very forms of ice-crystals that are observed in snow-figures or in hoar-frost. I have hit upon a method for producing them in an equally simple and satisfactory way. If a glass tube (with a length of one or two decim. and four or five millim. wide) has by means of the blowpipe one of its ends reduced to a diameter of two millim., some fibrous matter, as loose cotton-wool or gun-cotton, &c., must be introduced into this part, in such a way that many single fibres protrude out of the tube. They form the lower part when the tube is now fixed in a vertical position, and some sulphuric ether dropped in through the upper end in sufficient quantity to keep the fibrous substance moist, but not enough to run over. An active evaporation favoured by a comparatively large surface and the radiation from a multitude of points, sets in immediately, and within a minute ice-crystals, as a deposit from the atmospheric moisture, are seen growing in all directions on the fibres, imitating exactly the snow-figures. If very small quantities of ether are now continually supplied, a group of crystals and needles, sometimes to a length of two centim., is readily obtained, affording, when projected on a screen, a very elegant experiment which is rapidly going on and is successful even at a surrounding temperature of 13° C.

The way by which these crystals are here obtained may elucidate the question on the formation of the ice-crystals observed by the Duke of Argyll and recently discussed in NATURE. I think that the ligneous substance, from its rotten condition, presents an innumerable quantity of very thin fibres, cooling after sunset rapidly by radiation, and their surfaces, getting to a temperature beneath the freezing-point, cause the vapour of water, with which the surrounding atmosphere becomes now surcharged, to be slowly deposited in the crystallised form exactly as in the above experiment. The crystals, ending in needles and sharp points, continue to cool by radiation, and therefore increase at their extremities, till their length is sufficient to have gravity exerting its influence in curling them round the bark.

The Hague, Holland, March L. BLEEKRODE

Cloud Classification

THOSE who have long taken an interest in the subject of the classification of the clouds, will heartily congratulate themselves that this study is again resuming a fair share of the attention of meteorologists, and is likely to be more fully discussed than it has been for many years.

Luke Howard's Classification was, as far as he knew at the time, a first attempt to introduce order into fields of observation, then almost untried by scientific men. No one could suppose that it would at once exhaust the whole subject and be incapable of either extension or modification by later observers who possess the advantage of a much more mature stage of the science of meteorology. Still I may be allowed, without prejudging the result of the present discussion, to suggest one or two practical cautions to those who may be taking the subject in hand.

Firstly—Luke Howard's nomenclature of clouds has, since his time, been passed from hand to hand by a great number of observers, many of whom have apparently never taken much trouble to ascertain what he really intended to define by certain names, or what were the principles on which the classification was based. Therefore, before too readily finding fault either with the names or the original application of them, it might be well to give somewhat thoughtful study to the very carefully worded descriptions and definitions in Luke Howard's own work on the subject.[1]

Secondly—Clouds are by their very nature liable to frequent changes from one class to another, during which they must pass, more or less rapidly, through intermediate forms. If an attempt is made to classify all these temporary and intermediate varieties, the science will become rather unusually complicated. Were the same principle carried out in other branches of observation we should, for example, have to classify the *tadpole* as an important separate variety of the batrachians.

In conclusion I may remind the observer of the advice of Goethe in his remarks about Howard's nomenclature[2]—which advice is as applicable now as when it was first written—"Not to allow himself to be led astray by the occurrence of certain indistinct appearances, but to practice himself in referring the same to the main rules (or classes) under which they come."

Walthamstow, March ELIOT HOWARD

Diatoms in the London Clay

SINCE you were good enough to allow me to announce in NATURE, the discovery of diatoms in the London clay, I have been able to trace the band in which they occur throughout the whole extent of the London clay in East Kent—and at one spot in Mid-Kent. In continuing the inquiry, with vol. iv. of the *Memoirs* of the Geological Survey for my guide, I have found that sections that were visible when that valuable work was published are now overgrown or have been removed. Under these circumstances will you allow me to ask your readers for information as to places where tolerably fresh sections of the lower part of the London clay can be seen, especially at or near the northern and the western outcrop of the formation?

As regards the eastern part of Kent, the investigation is complete, and therefore no correspondence need take place respecting sections in that district.

Also I should like to have information as to any wells in course of being sunk in any part of the London basin.

I may observe that I have invariably found these fossil diatoms only in clay of a uniform dark slate colour, that dries out dark grey, and has a tendency to lamination.

On splitting open a fresh piece of clay, the diatoms, if present, are easily seen with the help of a pocket lens, as shining specks, and if plentiful their metallic lustre is evident to the unassisted eye.

W. H. SHRUBSOLE

Meteor

YESTERDAY evening, when observing the zodiacal light, in order to get its limits among the stars, I remarked a fire-ball in the same direction, which may have been perceived also in England, where it was seen, perhaps, near the zenith. I give you the elements of my observation to be published in your journal.

Mean local time 7h. 20m. evening, March 3.

Direction of the apparent path from the width of the arc $\mu - \xi_2$ *Ceti* towards the width of the arc γ *Ceti* – α *Piscium*.

The beginning was very small, but towards the end the brightness increased very quickly, and the phenomenon ended

[1] Essay on the Modifications of Clouds, by Luke Howard, F.R.S. Third edition, with plates. (Churchill, 1865.)

[2] Quoted in preface to third edition of the essay, &c.